JOURNAL
OF THE
WASHINGTON ACADEMY OF SCIENCES

VOL. 10 APRIL 19, 1920 No. 8

CHEMISTRY.—*Determining soil acidity and alkalinity by indicators in the field.*[1] EDGAR T. WHERRY, Washington, D. C.

About a year ago the writer[2] recorded a series of tests on soil acidity and alkalinity made by the indicator method[3] in the laboratory, the soil samples having been collected from stations where forty species of native orchids grew. These samples exhibited such characteristic differences in reaction that it seemed worth while to study other groups of plants in a similar manner. The bringing in of samples of soil in sufficient number to make the results significant would have involved, however, carrying numerous containers on field trips. It was, therefore, decided to work out a method for applying the tests in the field, so that only a few bottles of indicator solutions would have to be carried along.

The following outfit proved to fill the requirements.[4] First, a rectangular box about 3.5 × 5 × 9 cm. in dimensions. In the box, six vials for the indicators, 1.5 × 5.5 cm., capacity 8 cc., each provided with a cork or rubber stopper, into which is inserted a glass rod flush with the top of the stopper, and extending nearly to the bottom of the vial; to prevent undue compression upon inserting the stoppers, a groove may be cut in the side of each, so as to reach nearly to the lip of the vial. Then, three or four vials in which to extract the soils, about

[1] Received February 5, 1920.

[2] This JOURNAL 8: 589. 1918.

[3] CLARK and LUBS. Journ. Bacteriology 2: 1. 1917. GILLESPIE. This JOURNAL 6: 7. 1916.

[4] Sets of indicators similar to that here described are for sale by the La Motte Chemical Products Co., 13 W. Saratoga St., Baltimore, Md.

2 × 5 cm., made of heavy glass, to prevent undue breakage; a container for water, which may conveniently be a screw-capped jar holding 200 cc. or more, or an aluminum canteen: and a pipette, most simply constructed of two pieces of glass tubing a few cm. in length, connected by a rubber tube.

The six indicators which have proved most satisfactory in work with soils are: bromphenol blue, bromcresol purple, bromthymol blue, phenol red, methyl red, and *o*-cresolphthalein or phenolphthalein. The first three are used, as recommended by Clark and Lubs[5], in about a 1 per cent solution in water, titrated with dilute sodium hydroxide to their intermediate colors; and the phenol red in a 0.5 per cent solution similarly titrated. The methyl red and phenolphthalein are used as 0.02 per cent solutions in 50 per cent alcohol. It should be noted here that litmus paper, which is often recommended for testing soil reaction, is much less sensitive than the above indicators, and may give misleading results.[6]

Most of these indicators are dichroic, showing different colors as viewed by reflected and by transmitted light, and in the writer's paper, above referred to, several of their colors were rather inaptly characterized. In the new table here the former descriptions have been improved upon, the colors given being those produced by adding a drop of each indicator solution to a few cc. each of buffer solutions with different reactions, as seen through a 1 cm. layer against a white background. It has also seemed desirable to add the numbers assigned to the various colors in Ridgway's *Color Standards;* although in two cases, bromophenol blue in liquid of specific acidity 1000, and bromcresol purple in that of specific acidity 10, the colors are nondescript and cannot be accurately placed.

The special terms used in this table to describe the reactions have recently been defined by the writer.[7] By way of summary it may be stated here that the specific acidity is the amount of acid present in a given solution, as measured by hydrogen ion,

[5] Journ. Bacteriology 2: 135. 1917.
[6] Gillespie and Wise. Journ. Amer. Chem. Soc. 40: 796. 1918.
[7] This Journal 9: 305. 1919.

with reference to that of water as a unit.[8] Correspondingly, specific alkalinity is the amount of alkali, as measured by the hydroxyl ion, the unit being the same. Specific acidity and alkalinity numbers can be readily transposed into P_H values by anyone preferring that mode of statement. Find the power of 10 corresponding to the number; if acid, subtract from 7; if alkaline, add 7; the result is the P_H.

Under the most favorable conditions it is possible by the indicator method to measure acidity and alkalinity with much greater precision than is here attempted. By treating the indicators with buffer solutions of known ionic concentration, many hues intermediate between those here tabulated can be distinguished. On comparing the colors thus produced with those developed by mixing clarified soil extracts with the same indicators, specific acidities differing by a factor of $\sqrt[5]{10}$ or 1.59 ($P_H = 0.2$) can be recognized. In the field, where it is inconvenient to carry buffer solutions to prepare standards for comparison, and where the turbidity of soil extracts is difficult to remove, it is impracticable to work closer than values differing by a factor of $\sqrt{10}$ or 3.16 ($P_H = 0.5$) which is rounded off for simplicity to 3 +. This degree of precision is, however, entirely adequate for the purpose in view, for it has been repeatedly found that from one to another plant of the same species, or indeed, from one to another root on the same individual, separate observations of reaction may differ by a factor of 10 or more. To give a specific example, a plant of *Rhododendron maximum* growing in glacial drift near Williamsport, Pennsylvania, was found to have some of its roots in soil with a specific acidity of 1 (neutral), and other roots in soils with acidities of 3, 10 and 30. It seems obvious that nothing would be gained by measuring the acidity on any one of these to a high degree of precision, when the soils around the plant as a whole varied by a factor of 30.

[8] Only acidity in this sense is considered here; the effect variously termed latent, potential or negative acidity, and often shrouded in mystery by writers who fail to appreciate the significance of adsorption and other physical-chemical phenomena, has no bearing on the problem in hand.

TABLE I.

Classification of Soil Reactions and Indicators.

Specific reactions.....	← Acid							Neutral	Alkaline →						
	3000+	1000	300+	100	30+	10	3+	1	3+	10	30+	100	300+	1000	3000+
P_H values.....	3.5	4.0	4.5	5.0	5.5	6.0	6.5	7.0	7.5	8.0	8.5	9.0	9.5	10.0	10.5
Descriptive terms.....	superacid		mediacid		subacid		minimacid	minimalkaline		subalkaline		medialkaline		superalkaline	
							circumneutral								
Typical soils...	← bog-peat →				← leaf-mold →								← "alkali"-soil →		
			← upland-peat →					← limestone-soil →							
Bromphenol blue.....	dull green, 23k	brown-green	violet, 59 →												
Methyl red.....		← violet-red, 67		red, 1	orange, 11	yellow, 23 →									
Bromcresol purple.....				← yellow 21	brown-green	violet-red, 69'''		purple, 63 →							
Bromthymol blue.....					← yellow, 21	green-yellow 27		green, 35	blue, 49 →						
Phenol red.....						← yellow, 23		orange-ylw. 17	orange 11	red 1	violet-red, 69 →				
Phenol-phthalein....									← white		pale pink, 69 f	deep pink, 69 d	violet-red, 67 →		

In accordance with the above considerations, a simplification of the procedure previously recommended has been adopted; modifications may still be desirable in special cases. But before giving the directions, a word should be added concerning the water used for mixing with the soil. If calcium bicarbonate is present in this water, the soil acidity will be diminished; while if neutral salts, such as sodium chloride, and especially calcium sulfate, are present in any considerable amount, the acidity will be appreciably increased. The former effect is a direct neutralization; but the latter is due to the fact that the clay and the humus[9] in the soil adsorb the basic elements from neutral salts, and set the acid free.[10] In the laboratory, distilled water can be used, and to attain the greatest precision, air freed from carbon dioxide can be blown through it until it reacts quite neutral; when one is traveling, distilled water can usually be purchased from a drug store, and will give satisfactory results without special purification. In the wilds the best that can be done is to obtain spring or well water rising through rocks as free as possible from soluble constituents—such rocks as sandstone, shale or schist. In calcareous regions it may be necessary to test waters from one source after another until a sample is found which reacts neutral—is colored green by a drop of bromthymol blue indicator—and to arrange the trip so that the water supply can from time to time be replenished from this source.

With these points in mind, the following approximate directions have been drawn up:

A sample of soil a gram or two in weight is shaken from living roots into an empty vial, and 5 cc. of the most nearly neutral and salt-free water available is added, the vial being shaken well to insure complete mixing. After the soil and water are thoroughly mixed, the solid matter may be compacted with a glass rod or a stick, and the vial then supported at an angle of

[9] GILLESPIE and WISE, *op. cit.*

[10] This is, of course, the reason that the so-called lime-requirement methods in which a neutral salt solution is mixed with a soil yield so much higher results than can be obtained by direct titration of water extracts of the soils.

45° and allowed to stand until the bulk of the suspended matter has settled. The more or less clear liquid is then decanted or pipetted off into another vial, a drop or two of bromthymol blue or one of the other indicators, the color changes of which occur near the neutral portion of the table, are added, and the color assumed is noted. If either of the extreme colors is shown, the process is repeated with the indicator whose color changes come next in the corresponding direction; and this is continued until either an intermediate color of one indicator, or opposing extremes of two overlapping ones, are obtained, whereupon the specific acidity or alkalinity can be read off from the table.

The more turbid the liquid, the more indicator must be added, and the less certain are the results obtained. The turbidity can, of course, be removed by the addition of coagulating agents or by filtration through paper; but it is essential to make certain that these do not in themselves show an acid or an alkaline reaction. The most satisfactory results of all can be obtained by running a quantity of the soil through a paper filter until two successive portions yield the same value when tested with indicators. But such procedures are more suited to laboratory than to field studies, and after a little experience one can tell the indicator color-change with certainty, even in the presence of considerable brown mud.

To illustrate the procedure followed in actual practice, two typical cases encountered by the writer may be cited here.

(1) A black soil in pockets in limestone rock, supporting spleenwort ferns, was treated as above, and on testing the soil extract with bromthymol blue indicator, a strong blue color was obtained; reference to the table showed that the reaction must be alkaline, and the value of specific alkalinity 3 or more (P_H = 7.5). The process was repeated with the indicator the color changes of which lay next toward the alkaline side of the table, namely, phenol red. With this indicator a clear red color was obtained, showing the reaction to be actually specific alkalinity 10 (P_H = 8.0).

(2) Soil from a dry blueberry thicket was tested, and, since upland peat is usually distinctly acid, the first indicator tried

was bromcresol purple, the color changes of which occur just to the acid side of the neutral point; with this indicator a yellow color was obtained, indicating a specific acidity of at least 30. The soil was accordingly tried again with methyl red, which lies next toward the acid side, and this gave a violet-red color, corresponding to a specific acidity of 300 or more. It was accordingly necessary to try an indicator working at still higher acidities, namely bromphenol blue; and this yielded a violet color, indicating 300 or less. The last two indicators agreed, then, in fixing the reaction of this soil as: specific acidity 300 ($P_H = 4.5$).

In spite of certain limitations, this method is capable of giving definite information as to soil reaction in many cases. And the results obtained by the writer on a number of species of native plants, to be described shortly in other communications, have been of such significance that the method is now published for the benefit of students of plant distribution and others interested in soil acidity and alkalinity.

RADIOTELEGRAPHY.—*Musical reception with continuous waves without local oscillations.* L. W. AUSTIN, U. S. Naval Radio Research Laboratory.

The principle of what is now known as the slipping contact detector or ticker, was first applied to the detection of direct currents with the telephone by the author in 1900[1] and later applied to the reception of radio signals in 1906.[2]

This does not in general give a musical note in reception either for damped or continuous oscillation, on account of the irregularity of the contacts. If, however, a toothed wheel or any equivalent contact maker, such as is shown in figure 1, be provided with a brush bearing on the face of the wheel or axle in such a way as to produce a steady contact, while a second brush is adjusted so as to touch the teeth, musical reception can be obtained with continuous oscillations. For this purpose the alternating E. M. F. is impressed at DE and the wheel rotated at such a speed that the contacts of the brush E are made with

[1] Phys. Rev. 11: Aug., 1900.

[2] This JOURNAL 1: 6. 1911. Physik. Zeitsch. 12: 867. 1911.

the teeth at a frequency a little greater or less than the frequency of the applied voltage. Under these conditions an alternating current will flow in the telephones of a frequency equal to the difference in frequencies of the contacts and the applied E. M. F. The process is shown in figure 2 where the dots represent the contact points and the broken line the telephone current, the ripples being smoothed out by the reactance of the telephones. The resulting tone is not strictly a beat tone, although the result is exactly the same as though true beats had been produced. This device produces musical continuous wave reception by mechanical instead of by electrical means as in the Fessenden heterodyne.

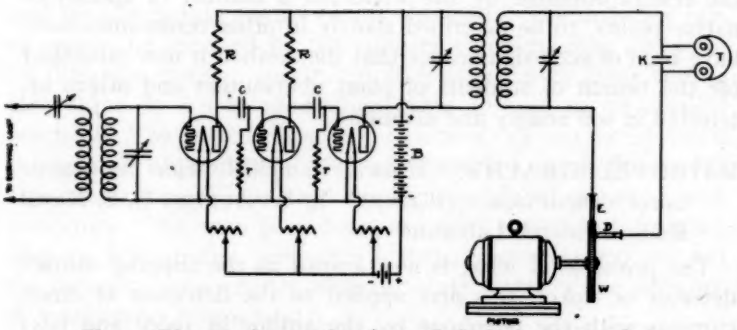

Fig. 1.—Diagram of connections for toothed wheel contact maker.

In 1913, R. Goldsmith devised the first practical application of this principle in his tone wheel (U. S. Patent No. 1087113, Feb. 17, 1914), although the circuits shown in the patent were somewhat more complicated. It was used for some time with the simple circuit described above, both at Arlington and Tuckerton in 1914. While entirely successful as a receiver in long distance continuous wave communication, it was less sensitive and less adaptable than the oscillating vacuum tube introduced in 1914 and was, therefore, generally superseded by it.

Recently the Research Laboratory has again taken up the study of the simplified tone wheel or musical contact maker

which was interrupted in 1914, the object of the present work being the determination of its sensibility, the law of response, and its general applicability to modern receiving conditions, especially with amplifiers.

The contact maker used in the experiments (figure 1) was an old tone wheel having a steel disk about 28 cm. in diameter and 754 teeth (with brushes arranged as described above). With

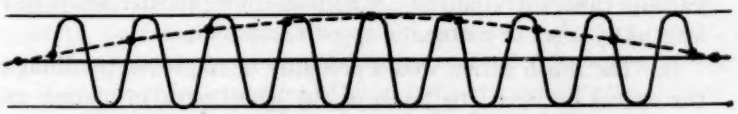

Fig. 2.—Diagram showing contact points and telephone current.

this a contact frequency corresponding to a wave length of 10,000 meters is obtained at a speed of approximately 2400 R. P. M., the power consumed by the motor being about 1/10 H. P. In order to reduce disturbances in the telephones, the steel disk was insulated from the motor, and the frame connected to ground. With the motor run from a storage battery no trouble was experienced in keeping a practically constant speed with which the European stations could be read for hours at a time without speed adjustment. For unsteady sources of power, a speed regulator is of course required. The telephones employed were Baldwin's of 2000 ohms resistance. Figure 1 shows the circuits employed with radio frequency amplification.

Comparisons of the sensibility of the oscillating vacuum tube and tone wheel without amplification were made on Annapolis changing the strength of the signal from 10 audibility to several thousand by inserting resistance in the receiving loop, and also in some experiments by varying the main capacity. The results were as follows:

(1) The sensibility of the tone wheel without amplification varies from 1/6 to 1/3 of the sensibility of the oscillating vacuum tube, depending upon the tone and brush adjustment.

(2) The law of response between telephone current and radio frequency current is linear as in the oscillating vacuum tube.

The tone wheel has, therefore, all the advantages of the latter in keeping out interference and static. In fact, it seems somewhat superior in keeping out strong interference.

(3) It may be used either with radio or audio frequency amplifiers. Radio frequency amplification is in general to be preferred on account of possible induction and brush noise.

(4) While less adaptable to wave length changing than the vacuum tube, this could be accomplished for predetermined wave lengths by a set of automatic speed regulators.

(5) The brush action would probably be improved by filling in the spaces between the teeth of the wheel with insulating material so as to present a smooth surface to the brush.

METEOROLOGY.—*The use of solar radiation measurements for weather forecasting in Argentina.*[1] C. G. ABBOT, Smithsonian Institution.

Mr. H. H. Clayton, the well-known American meteorologist now in charge of the forecast division of the Meteorological Service of Argentina, has employed solar radiation observations for more than a year as a forecasting element. I am not informed as to the precise details of his methods. In a conversation I held with him in June, 1919, at La Quiaca, Argentina, he told me that he has maintained for years an impartial quantitative mathematical record of both the success and failure of the Argentine official weather forecasts, and that this record showed marked and considerable gain in forecasting from the time of the introduction of this new element. He stated to me quantitatively the results before and after this event, but as I do not fully understand his system of accounting, I will not venture to repeat them.

This new departure rests on the fact that our sun is a variable star. This result was reached by the Smithsonian Institution in its investigations of the intensity of solar radiation. For nearly 15 years the Smithsonian Astrophysical Observatory has maintained a solar radiation observing station at Mount Wil-

[1] Presented before the Washington Academy of Sciences on January 29, 1920.

son, California. This station is usually occupied from May to November. Its main investigation comprises spectro-bolometric determinations of the so-called "solar constant of radiation" after the general method of Langley. This term designates the amount of heat per square centimeter per minute which would be produced by completely absorbing the sun's radiation outside our atmosphere at the earth's mean solar distance.

Early results indicated that this quantity is not really a constant, but varies over a range of several per cent, both from year to year and in short irregular periods of days or weeks. Confirmation of these results has been secured in many ways, so that now there remains, I think, but one possible explanation of the phenomenon other than that the sun itself varies in its emission by several per cent from time to time. This alternative possibility is that atmospheric changes occur simultaneously over the whole earth which lead to variable erroneous determinations of the so-called "solar constant," and that the errors thus produced are nearly equal and introduce apparent variations in the same sense, however far apart the two simultaneously observing stations may be. I believe it is easier to admit that the sun itself is variable as supposed. Other irregularly variable stars are numerous. There is no reason why the sun, too, may not be variable.

It is true that recently Dr. Guthnick of the Berlin-Babelsberg Observatory has made a good many photo-electric measurements of the relative brightness of the planets Jupiter and Saturn compared to reference stars. His results thus far have not confirmed the variability we have found. But they have not disproved it. His observations have, heretofore, seldom been taken on coincident days with ours. This inconvenience we expect will be remedied for the year 1920. Guthnick's results show a range of several per cent. This may be in part really solar. If no solar variation was suspected one would attribute it to experimental error. If it should prove that Guthnick in the future finds variations in planetary brightness similar to our solar changes but not coincident in time, we must recall that the planets generally lie in different directions. If the solar

changes are due to what we might call solar cloudiness, the effects should not occur coincidentally in different directions from the sun.

One of the most convincing proofs of the essential soundness of the measurements which indicate solar variability comes from Clayton's investigations of terrestrial temperatures. These have been published in Argentina, but also simultaneously by the Smithsonian Institution in its Miscellaneous Collections.[2]

In his first paper Clayton discusses the departures from normal temperatures for about 30 stations widely distributed over the earth, as related to the "solar constant" values determined on Mount Wilson. He does this largely by the mathematical method of correlations, but not wholly so.

If two quantities vary in the same sense wholly dependently the one on the other, as for instance the lengths of the radii and circumferences of circles, they are said to have a correlation coefficient of $+1$. If the dependence is complete but the variations occur in opposite senses, as the widths and lengths of rectangles of constant area, the correlation coefficient is -1. Between these limits there are all magnitudes depending on degrees of dependency between the two variables. If entirely unrelated, the coefficient is zero. In this method of investigation, quite common in agriculture, eugenics and other sciences, we must take account especially of the algebraic sign of the coefficients and of their magnitude, compared to their probable errors, in forming an opinion of the nature and degree of dependence of the qualities examined.

As has been said, the Mount Wilson solar observations indicated irregularly recurring variations sometimes reaching extremes of 10 per cent in the solar heat available to warm the earth. These changes often ran their course in a week or ten days. Ranges of 10 per cent are rare but those of 2 or 3 per cent are common.

Clayton found that coefficients of correlation ranging from $+0.54$ to -0.50 occurred as between solar and temperature changes. His studies covered not only the day of the solar

[2] Vol. 68, No. 3, and Vol. 71, No. 3.

observation itself, but the five days next following. He found that the largest temperature effects occurred generally from the third to the fifth day after the solar event.

Correlation coefficients numerically as large as 0.50 are practically always certain evidence of strong dependence between the variables. Clayton, therefore, very justly concluded that for some regions, at least, the observed variations of the sun so greatly influence temperatures that the effects are not masked by terrestrial influences. Furthermore the delay of several days between the solar cause and the terrestrial effect gave promise for useful forecasting.

A remarkable result is the opposing signs of correlation. An increase of solar radiation is attended at some stations by positive and at others by negative temperature departures. Clayton marked his stations on a world map and seemed to find that while in the tropics and polar zones positive correlations prevail, negative ones are found generally in both north and south temperate zones.

These early results of Clayton's seemed so interesting and promising that the Smithsonian Institution appeared justified in establishing a new solar radiation observing station in the most cloudless available region in the world, in order to furnish solar values regularly through the entire year. After a disappointing expedition to North Carolina, a region chosen only because of war conditions, the station was located in July, 1918, at Calama, Chile. Two observers, Mr. A. F. Moore, Director, and Mr. L. H. Abbot, Assistant, have occupied this desert station continuously up to the present time. Their zeal and success have been remarkable. The station has not quite satisfied our hopes for cloudlessness but determinations of considerable weight have been made on about 75 per cent of all days since July 27, 1918.

In the meantime, Mr. Clayton and his colleagues in Argentina have diligently continued their computations of the terrestrial effects produced by solar variations. The results they reached, up to June, 1919, have just been published by the Smithsonian Institution.[3] They are indeed remarkable, though confined al-

[3] Misc. Coll. Vol. 71, No. 3.

most exclusively to Argentine weather stations, and mainly to Buenos Aires.

Clayton carries on the study of what happens after a change in solar radiation for many days, sometimes even 40 days after the event. This leads to the surprising result that the largest effects come not 3 days, but even 10 days and 17 days after the event. As he has shown this result clearly by the ordinary method of graphical comparison, not involving mathematical correlation coefficients, I am able to show you the result with

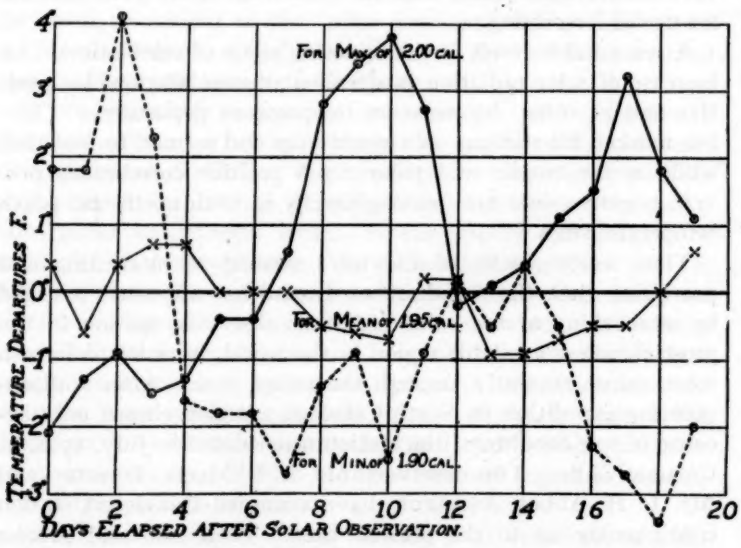

Fig. 1.—Curves showing departures from mean temperatures at Buenos Aires.

perfect simplicity in figure 1, which I have re-drawn on the Fahrenheit scale from his table II. Three curves are shown representing the average progress of the departures from mean temperatures at Buenos Aires. These cover the 10 days next following dates when the "solar constant" was determined as 2.00, 1.95, and 1.90 calories per square centimeter per minute, respectively. The curves give the mean results of the years

1913, 1914, 1915 and 1916,[4] for the months May to November. The solar observations were made at Mount Wilson. Note the pronounced and opposite courses of the temperature departures after high and low solar radiation values, and the nearly normal temperature following mean solar values. Note, too, that the temperature departures are not small. They amount to several whole degrees, not mere tenths of degrees. In fact on the second, tenth, and seventeenth day after the event, the temperature departure after the high solar radiation differs in each case more than 6° F. from the corresponding departure after low values. This corresponds to 5 per cent change in the sun's radiation. Meteorologists need not be reminded that 6° F. change in the mean temperature of a whole day is not trifling. People in general would also very easily recognize a day whose mean temperature was 90° from one of 84° F., or one of 10° F. from one of 16° F.

Perhaps equally or more important in Argentina, where there is hardly adequate rainfall, are Clayton's results on the dependence of precipitation on solar radiation, if confirmed.[5] He finds that heavy rains are apt to occur from three to five days after large decreases of the solar radiation. He shows this result by table 1.

At times of nearly stationary solar intensities there seems to be practically no precipitation in these cities of Argentina. In a few instances precipitation follows large increases of radiation. But almost universally great decreases of solar radiation are followed in from 3 to 5 days by heavy precipitation. Such information is of great value for vineyard growers and agriculturists in other lines if it proves to be well founded.

These are but specially striking samples of the results which Clayton lays before us. He has discussed all of the Mount Wilson and Calama solar observations for their bearing on the

[4] Clayton did not have solar results of 1916 and 1917 available for study when he obtained these results.

[5] Clayton's statement is not specific at this point, but I think his precipitation table depends only on a few months of observation, not on the mean of 4 years like the temperature data.

weather of Argentina. His studies have great variety. They deal especially with the discovery of periodicities in the phenomena which he treated by harmonic analyses as well as by simpler methods.

TABLE 1.

RELATION BETWEEN RAINFALL AND "SOLAR CONSTANT."

	Average daily rainfall in mm.						
	3 to 4 days later			5 days later			
Solar changes in calories	Mar del Plata	Buenos Aires	Cordoba	Parana	Corrientes	Tucuman	Mean
+0.050 to +0.070.....	0	9	3	0	2	2	2.7
+0.030 to +0.050.....	0	0	0	0	0	2	0.3
+0.010 to +0.030.....	6	6	0	0	2	6	3.3
—0.010 to —0.030.....	0	2	5	5	1	10	3.8
—0.030 to —0.050.....	8	11	4	3	8	14	8.0
—0.050 to —0.070.....	4	7	2	15	17	12	9.5

In December, 1918, Clayton began to employ the results furnished by the Smithsonian observers at Calama, Chile, for actual forecasting. Fully convinced of the value of such data, Prof. C. C. Wiggin, Chief of the Argentina Weather Service, arranged for a daily telegraphic service from Calama to Buenos Aires. By their great skill and zeal, Messrs. Moore and Abbot, the Smithsonian observers at Calama, have completely reduced the "solar constant" value on each day of observation. They send a code telegram from Calama via Antofagasta and Valparaiso, Chile, to Buenos Aires on the evening of each observing day. This states the intensity of solar radiation outside our atmosphere, and the quality of the determination. The value is available in Buenos Aires for the forecasting on the following morning, within 24 hours of the time of observation.

Fortunately, during my visit at Calama in June, 1919, with the cooperation of the observer there, I was able to perfect a

new empirical method of "solar constant" determination, based upon data obtained by Langley's methods, but independent of gradual changes of transparency of the atmosphere during observations. Hitherto it has required several hours of uniform atmospheric transparency to enable us to make the proper observations. If the sky was growing clearer our result was too high, and if more hazy, too low. The new method of "solar constant" determination is based on the fact that the atmospheric transparency varies in an opposite sense to the variations of the brightness of the sky. Increased haziness means more reflecting surface to scatter the solar rays indirectly to the earth. At the same time it means more obstructing surface to cut off the direct solar beam. We have found accordingly that from measurements of the brightness of the sky near the sun it is possible to infer the atmosphere transmission coefficients at all wave lengths. In our new process all the observations can be made in 15 minutes, and the "solar constant" value can be completely worked out in a couple of hours. The following great advantages may be claimed for the new method: 1. Great saving of labor. 2. Possibility of making several independent determinations each day. 3. Greater accuracy because independent of the variability of the atmospheric transparency. 4. Availability on partly cloudy days. On the other hand the new method is empirical, and must be frequently checked against the old to make sure that no new atmospheric conditions have arisen to invalidate it.

In letters just received from Mr. Clayton, he states that his most recent studies have but increased his enthusiasm for the value of solar radiation observations in forecasting. He encloses curves showing a striking direct correspondence between the temperature departure for Buenos Aires and other South American cities in November and December of 1919, and the slightly antedating solar radiation changes. The direct temperature effects lag from two to three days behind the solar fluctuations.

We now come to the most interesting and puzzling feature of these new discoveries. It is that the dependence between solar

variations and changes of terrestrial temperatures at Buenos Aires occur in opposite senses at different seasons of the year. From October to February, inclusive, low values of solar radiation are followed for several days by negative temperature departures from the normal, and the reverse is true for the months March to September, inclusive. Naturally the intervals of transition in March and October from one of these conditions to the other are periods when the solar radiation results are at present of little value for forecasting purposes. Besides this inconvenience, it occasionally happens in the midst of one of the long periods of positive or of negative correlation, that a sporadic regression to the other type of correlation will occur to mar the forecast. It may be that with further investigation these things will be understood. Something about the prevailing direction of the winds or of the condition of the upper air may come to light to serve as a basis of prediction whether the correlation will be positive or negative at a given epoch.

It has been mentioned that Mr. Clayton discovered in his early studies that in different parts of the world correlations of opposite algebraic signs between solar radiation and temperatures prevail simultaneously. It must, therefore, be tha tthere are geographical regions of transition, as well as transition time epochs at a single region. Hence the new means of forecasting cannot yet be regarded as either simple or fully satisfactory. Much investigation must be made before they take established rank in meteorology. Enough has been done to show that there is promise. Before the promise can come to fruition we must have continuous daily records of well determined solar constant values. These cannot be secured with the means now available. The solar radiation station at Mount Wilson is not occupied more than six months per year, and never yields more than 130 values of the "solar constant" in that period. Of those not all are good. The station at Calama yields about 250 good values per annum now that the new method of observing is adopted. These are the only stations of the kind in the world. There should be two or three others, widely separated in the most cloudless regions available. I have in mind Egypt, Southern California and Middle Australia.

The cost of a solar radiation station need not exceed $25,000, and its upkeep $10,000 per annum. Since the accuracy of the determinations of the solar variations would be enhanced by uniformity in the methods of observing, it is quite desirable that the measurements at the several stations should be made under a common control and direction.

The methods of observing and reducing have been devised and perfected at the Smithsonian Institution but they would very willingly be communicated to any international organization which was prepared to take up measurements of the "solar constant." On the other hand, if the Smithsonian Institution had the means, preferably $1,000,000, to devote to the subject, it would be practical for the Institution to carry on "solar constant" determinations in perpetuity in such a manner as to afford a satisfactory groundwork for any application of them which meteorologists may wish to make hereafter.

PROCEEDINGS OF THE ACADEMY AND AFFILIATED SOCIETIES

WASHINGTON ACADEMY OF SCIENCES

139TH MEETING

The 139th meeting of the ACADEMY was held in the Assembly Hall of the Cosmos Club at 8.15 p.m., on Thursday, January 29, 1920; President ALSBERG presided. Dr. C. G. ABBOT, Director of the Astrophysical Observatory, Smithsonian Institution, delivered an illustrated lecture on *The use of solar radiation measurements in weather forecasting in Argentina.* The substance of the lecture is published in this number of the JOURNAL.

Discussion. Professor C. F. MARVIN, Chief of the Weather Bureau, presented the following discussion:

We must all admire and commend in the highest terms the persistent and conscientious effort which Mr. Clayton has expended in carrying forward for several years the tedious studies he has executed to exhibit a correlation between daily and short-period fluctuations in observed values of intensities of solar radiation and terrestrial temperature and other phenomena of weather. I wish especially to commend in the highest terms the splendid work done by the Astrophysical Observatory of the Smithsonian Institution under the able directorship of Dr. Abbot in perfecting methods and apparatus for the exact measurement of solar radiation intensities, and in securing almost daily values thereof. I do not know of any one element of observation possessing greater fundamental importance to theoretical meteorology than that of the intensity of solar radiation. Dr. Abbot's investigations command our unqualified appreciation and his work deserves to be encouraged and extended as far as possible, purely on the basis of the great value of the work itself.

With reference to Mr. Clayton's findings, I feel compelled to say I cannot at present join with Dr. Abbot in his seeming enthusiastic acceptance of the former's views. Mr. Clayton's latest paper has been in my hands only a few days and has received but a very hasty examination. The hesitation I feel in accepting its conclusions is based on certain general principles and convictions and may be overcome by a critical and detailed study which such a paper must receive before its merits or faults are fully disclosed.

It is obviously impossible, in the few minutes available to me this evening, to attempt to discuss any details of this highly complex and intricate problem—I must confine myself to a very few fundamental considerations and convictions.

There are at least three basic questions which must be answered in the affirmative before we can accept Mr. Clayton's views; these are: (1) Are they in accord with the theoretical, physical or scientific principles which we believe govern the phenomena? (2) Are the data employed so entirely free from systematic errors as to be practically unimpeachable for the purposes for which Mr. Clayton has used them? (3) Are the methods of analysis and treatment of the data sufficiently *conclusive* to command confidence and carry conviction?

From my understanding of the whole question at the present time, I am obliged to entertain a negative reply to each one of these questions to such an extent as to make me believe Mr. Clayton's proposition is not yet conclusively proven. (1) In 1916, I was called upon to decide concerning the merits of another case of correlation between terrestrial weather and certain well-known solar features; in this case the spottedness of the sun. At that time I formulated a physical principle which seems to me to serve as a guide in questions of this kind. This was published in the Monthly Weather Review for January, 1919, and is as follows:

"Meteorologists have long been accustomed to ascribe practically all atmospheric motions, both local and general, to the gravitational flow resulting from the local and general contrasts of temperature over the surface of the earth. The atmosphere derives its heat, not directly from the sun, except to a small extent, but chiefly from the surface of the earth itself. The daily sequence of sunshine and darkness; the varied distribution of clear and cloudy skies; diversities of surface cover added to contrasts of land and water areas, including the phenomena of evaporation, condensation and precipitation; the cycle of the seasons, and above all the fluctuating but nevertheless perpetual contrasts of surface temperatures, ranging all the way from the heat of the tropics to the intense cold of the polar zones constitute a complex series of varied and changeable influences seemingly abundantly adequate to cause and explain every feature of our weather conditions, however changeable we may find them.

"These differences and contrasts on the one hand perpetually disturb the orderly arrangement of air densities and pressures demanded by gravity. The latter, on the other hand, as perpetually and continuously sets portions of the air in motion, in order to establish and maintain a state of equilibrium, which, however, is never attained, or rather we must clearly recognize that *the ceaseless complex changes in and motions of, our atmosphere represent in fact the only state of equilibrium possible between gravity on the one hand and solar heating of the earth on the other.*

"Seemingly with little regard for the considerations just mentioned, many have sought and still seek to ascribe terrestrial weather—that is to say, *all* the characteristic features of atmospheric variations—to *minor* features of solar activity, as, for example, to the spots and faculae of the sun or to its magnetic manifestations, or to the relatively small

and irregular fluctuations in the intensity of its thermal radiations, or to some of these variously in combination, etc.

"Even suppose these solar phenomena directly influence terrestrial weather in some way yet to be proved, is it not plainly most essential in detecting and analyzing cause and effect relations that we adequately segregate and make due allowance for the complex phenomena which clearly must result if solar insolation were perfectly constant and if the other manifestations of solar activity were entirely absent?

"Those who have been most ready to find convincing evidence of definite relations between terrestrial weather and minor features of solar activity have seemingly disregarded the obligation devolving upon them to make the segregation between the major and the minor influences. . .

"Variations in the intensity of thermal radiations from the sun must, of course, be reflected in terrestrial weather phenomena, but such reflected effects must stand in appropriate relation quantitatively to the variations themselves."

These considerations prevent me from concurring in Mr. Clayton's conclusions which he states in his own words as follows:

"The results of these researches have led me to believe: 1. That if there were no variation in solar radiation the atmospheric motions would establish a stable system with exchanges of air between equator and pole and between ocean and land, in which the only variation would be daily and annual changes set in operation by the relative motions of the earth and sun. 2. The existing abnormal changes, which we call weather, have their origin chiefly, if not entirely, in the variation of solar radiation."

Mr. Clayton's paper in no way defines what constitutes a "stable system" of atmospheric motions nor does it offer a direct proof of these conclusions. They are simply generalized inferences drawn by him from his investigations.

I realize that my own statement as well as that of Mr. Clayton is of a kind that it is difficult or impossible to prove or disprove. They are contradictory, however, and I leave them to your reflection as to their physical soundness.

(2) I am most reluctant to raise any question as to the final accuracy of Dr. Abbot's solar radiation values, because I know the conscientious care he has taken to eliminate systematic errors due to terrestrial causes. However, this is a matter about which we must have a definite answer, yes or no. Dr. Abbot fully recognizes the importance of this issue, and, in his introductory note to Clayton's paper, he states the question in this way:

"I now anticipate the question of the reader: . . . . . is it possible that the apparent variations of radiation were not truly solar, but were caused by changes in the transmissibility or other properties of the air which affected the solar radiation measurements in one way and the temperature and rainfall of the earth in another?"

The negative answer he offers to this question in the immediate context is based in part on very indirect evidence drawn wholly from Clayton's work itself. This is too much in the nature of reasoning in a circle to carry conviction and can not outweigh the very direct adverse evidence derived from a critical examination of the simultaneous data showing atmospheric transmission and solar radiation intensities. A study of this kind has been carried on at the Weather Bureau for the past several months by Mr. Clough, whom I regard as a most conscientious and astute student of questions of this nature, and I am unable to refute the results of the studies he has thus far shown me, and these seem to me to indicate that in at least some of the observations employed by Mr. Clayton the value of the solar constant comes out high for low atmospheric transmission, and the value is low for high transmission. This question of the accuracy of the data must be removed before Mr. Clayton's views can be established.

(3) Finally, I must express skepticism as to the conclusiveness of a demonstration resting mainly or alone on correlation coefficients and the comparison of somewhat similar curves. Correlation coefficients are quite meaningless without the probable error of the coefficient, and I notice the probable errors of the correlation coefficients have been completely omitted from Mr. Clayton's last account of his work. This makes it impossible to properly weigh the evidence submitted.

Every problem in the correlation of two variables can be graphically represented by a so-called dot chart in which the position of each dot with reference to the conventional coordinate axes represents the simultaneous values of the two variables. In nearly all problems of this kind with which I have any acquaintance the dots fall in a widely scattered "star cluster" sort of arrangement, signifying a large measure of inconsistency. The correlation coefficient serves simply to define the straight line of least-square best fit for the given cluster of dots. The probable error of the correlation coefficient is an index of the amount of scatter of the dots. In a great many cases the clusters of dots are nearly as broad as they are long, and the direction of the straight line of best fit in such cases is determined almost wholly by a small number of pairs of the variables which have extreme high or low values. The great bulk of the dots serve no other purpose than to fix the origin of the coordinate axis at the center of gravity of the system. We may use results of this sort with a high probable error as a basis for rough estimations, approximations, or even forecasts. But I cannot feel justified in accepting them as demonstrations of cause and effect relations.

Summarizing my views, I may say, first, I am not convinced of the entire physical soundness of Mr. Clayton's first and second conclusions stated. Second, I think it is still possible there may be some residual error in measurements of solar radiation intensities by which a portion of those values, sufficient to influence final conclusions, are systematically high with low atmospheric transmission and low with high

atmospheric transmission. Third, I am not convinced of the conclusiveness of Mr. Clayton's methods of analysis of the data employed.

In conclusion, I wish to make it very clear that my skepticism is not in the slightest degree directed against the work of the Astrophysical Observatory. Dr. Abbot's work should be supported and extended in the fullest possible manner purely on its own merits and on account of the importance of observations of intensity of solar radiation to general meteorology. For example, I wish we might have, and I am ready to recommend urgently that we have, say 12 stations, maintained throughout the world. These should be located as nearly as possible in four groups of three each, the three located on the same meridian of longitude, the four groups separated by approximately 90° of longitude. Such a system of stations would make possible simultaneous measurements of intensity every six hours. This proposal is submitted largely as a scientific desideratum. It is recognized that geographical and meteorological conditions operate in a very material way as obstacles to its complete realization.

Dr. W. J. HUMPHREYS, of the Weather Bureau, said that it is now quite certain that the sun is a variable star. Explosions, or something analogous thereto, cause spots in the sun, and naturally lead us to expect variability in its radiation such as is actually found in Dr. Abbot's measurements. It is not to be expected that more stations will disprove the variability of the sun; they will only establish more accurately its amount. Since all weather phenomena are the result of the sun's radiation, any change in that radiation must produce a corresponding change on the earth. The only question is as to the kind and magnitude of this change. The sun's radiation is partly reflected from the outer atmosphere, and partly absorbed, mainly in the lower levels, while the remainder is absorbed at the earth's surface. If the sun's variability is known, a rough computation can, therefore, be made of the kind of result to be expected. Abbot and Clayton have gone far enough to show that there is considerable value in the effort to correlate the variations in the solar constant and in the weather, and at least six stations should be established to carry forward the study of the solar constant.

All of Clayton's deductions, however, are not sound. What he calls a "stable system" would not necessarily result if there were no variation in solar radiation. A river with a constant supply of water and undisturbed at its mouth by tides may still be full of turbulent currents. The weather cannot be dependent *solely* on changes in the sun.

The location of Buenos Aires is unfortunate for an attempt to correlate weather and solar variation. An inland high-level station would be much better. The lower levels of the atmosphere near the sea are usually moist and dusty and do not respond as quickly as the high levels; variations in the wind direction, especially as between on and off shore, are also most disturbing near the coast.

Dr. C. F. BROOKS, of the Weather Bureau, referred to the difficulty which positive and negative correlations occurring at different seasons introduce into any attempt to use solar variations in forecasting. In a lecture before the ACADEMY in 1918 Nansen[1] showed how surface temperatures at different places on the earth varied either directly or oppositely with the sun spot numbers, and how the direction of variation changed from time to time, due evidently to shifts in the centers of action of the atmosphere. There are "fixed" centers of action, as at the Azores, which control seasonal weather; but it is the moving centers (the high and low pressure areas) that control our weather from day to day. If the actual positions of Highs and Lows be taken into consideration, we may find it possible to predict whether a given variation in the solar constant will have a positive or negative result at a particular place. Increasing the strength of a High, for instance, would have opposite effects on temperature at stations on the east and west sides of the High.

As an example of a probable correlation between solar and weather phenomena, the speaker called attention to six successive recurrences of abnormally high pressures somewhere in the United States or Canada in the fall and winter of 1917–1918; these recurred at about 27-day intervals, or about the synodic rotation period of the sun in low latitudes. The maxima occurred, however, at different places.

The time is perhaps not distant when the weather forecaster, having before him the existing and expected locations of the Highs and Lows, may be able to use solar constant data to predict most of the now inexplicable changes in the intensities of those centers.

Dr. L. A. BAUER, of the Department of Terrestrial Magnetism, Carnegie Institution, made special mention of the achievement of the Astrophysical Observatory in introducing new methods that so greatly diminished the time and labor of obtaining solar constant data. He hoped that a continuous record of the solar constant might some day be possible.

Dr. ABBOT stated his gratification that all the speakers had agreed on the need of additional stations to obtain more and better values of the intensity of solar radiation. The data available to Clayton were far from being completely satisfactory, for the measurements were frequently interfered with by cloudiness at both Mt. Wilson and Calama. Nevertheless, he believed the correlation of the radiation and temperature observations too striking to be avoided, whatever the theoretical application may be. Dr. Brooks' suggestion regarding the movement of action-centers may well account for Clayton's varying correlations. In reply to a question by Col. T. L. CASEY he pointed out that the average value of the solar constant has not changed in the right direction to account readily for the great difference between the winters of 1918–19 and 1919–20 in Washington, but that the weather depends to

[1] This JOURNAL 8: 135–138. 1918.

a great extent on the direction of wind, and the relation between solar constant and the direction of wind in Washington may be very complex. The solar constant for the period 1902–1912 averaged 1.93 and values as low as 1.85 appeared often. During the years 1914 to 1920 it averaged 1.95 and seldom fell as low as 1.90. In September, 1919, 1.93 was again the mean value, while for October, November and December it had risen to 1.96. Notwithstanding this recent increase we have a cold winter in the United States.

ROBERT B. SOSMAN, *Corresponding Secretary.*

SCIENTIFIC NOTES AND NEWS

MATTERS OF SCIENTIFIC INTEREST IN CONGRESS[1]

On February 19 Mr. TILSON introduced a joint resolution (H. J. Res. 299) "extending the life of the National Screw Thread Commission for a period of two years from March 21, 1920." The Committee on Coinage, Weights and Measures reported it in the House on February 24 (Rep. 671), and it was passed on March 1. The Senate passed the resolution on March 17.

This Commission was appointed under an act approved July 18, 1918, and consists of two representatives each from the Army and Navy, and four members nominated by the national engineering societies, with the Director of the Bureau of Standards as chairman. The Commission has investigated and formulated standards of commercial screw-thread practice which have been made accessible to engineers and manufacturers and have been tested in use. The life of the Commission is extended in order to give opportunity for minor modifications in the proposed standards before its final report is promulgated.

The bill for a tariff on scientific instruments, chemical glass and porcelain, and surgical and dental instruments[2] (H. R. 7785) had been in the hands of the Senate Committee on Finance for several months, and it had been agreed in the Committee that the dyestuffs bill (H. R. 8078) should have the right of way as the most important of the tariff bills. An attempt was, therefore, made by Mr. WATSON on February 25 to bring the dyestuffs bill before the Senate, but objections were made by Mr. POINDEXTER and others on the ground that the other tariff bills passed by the House (magnesite, scientific instruments, etc.) had not been reported and that dyestuffs had no right to special consideration. The result was that no action was taken on any of the bills at that date. Later, on March 4, the bill for a tariff on scientific instruments, etc., was reported in the Senate without amendment (Rep. 459) and recommended for passage. The report laid special stress on the testimony of Mr. J. M. ROBERTS, Secretary of the Scientific Apparatus Makers' Association, whose figures showed that a tariff of 60 per cent ad valorem would equalize Japanese and American costs of production on the six items quoted by him, with a maximum deviation of 8 per cent.

The Patent Office reform bills[3] (H. R. 5011, 5012 and 7010) were combined by the House Committee on Patents into a new bill (H. R. 11984) "To increase the force and salaries in the Patent Office, and for other purposes," which was introduced by Mr. NOLAN, and passed by the House on March 5.

[1] Preceding report: This JOURNAL 10: 148. 1920.

[2] See this JOURNAL 9: 389, 421, 562. 1919. 10: 149. 1920.

[3] This JOURNAL 9: 422. 1919.

The Senate Committee on Military Affairs finished in January its work on the Army reorganization bill (S. 3792) and in the bill as reported in the Senate on January 28 (Rep. 400) the Committee provided for a separate Chemical Warfare Service in the Army. 126 officers and 1200 enlisted men are provided in this Service. The corresponding House bill (H. R. 12775) which was reported February 26 (Rep. 680) provides 91 officers and 1500 men. During the debate on the bill in the House the Chemical Warfare paragraph was criticized as prescribing no duties for the Service, but no change was made. The bill was passed by the House on March 18.

A committee of the House made a tour of inspection to the Government nitrate plants near Muscle Shoals, Alabama, in the latter part of January, to obtain information relative to the feasibility of their private operation. Hearings on the Senate bill[4] to establish the United States Fixed Nitrogen Corporation (S. 3390, Mr. WADSWORTH, November 7, 1919) were begun before the Senate Committee on Agriculture in the latter part of March.

A new departure in weights and measures is embodied in S. 3943, introduced (by request) by Mr. KING, one of the Senators from Utah, on February 18. The bill is entitled: "A bill to establish the standard and decimal divisions of the weights, measures and coins of the United States." A similar bill, with minor changes, was introduced in the House by Mr. WELLING, a representative from Utah, on March 1 (H. R. 12850). The character of the plan may be indicated by the fact that the English foot is made the fundamental unit, with subdivisions into a "decimal inch" which is one-tenth of a foot, and a "common inch" which is one-twelfth of a foot. This plan of a multiple set of factors is carried through the system, which is intermittently octaval, decimal or duodecimal. The bills were referred to the Senate Committee on Standards, Weights and Measures, and the House Committee on Coinage, Weights and Measures, respectively. Editorial writers in the technical press seem in some doubt as to whether the bill is a serious proposal, a practical joke, or a "smoke screen" in anticipation of metric legislation.

Metric legislation has been compiled into the form of a bill, but has not yet been introduced, at this writing. Meanwhile, an extensive brief in opposition to the introduction of a "metric system bill" was submitted in March to the House Committee on Coinage, Weights and Measures, by the American Institute of Weights and Measures.

The report of the Joint Commission on Reclassification of Salaries[5] was presented to Congress on March 12, but at this writing no legislation based on the report has been introduced.

[4] The House bill is H. R. 10329. See this JOURNAL 9: 646. 1919.

[5] This JOURNAL 10: 148. 1920.

NOTES

The following program of papers was presented at the public meeting of the Board of Surveys and Maps at the Interior Department on March 9, 1920:

1. *Coordination of Government mapping and surveying through the Board of Surveys and Maps*, O. C. MERRILL, Chairman of Board. 2. *Report of Joint Committee of Non-Federal Agencies*, M. O. LEIGHTON, Chairman of National Service Committee, Engineering Council. 3. *Need for a general topographic map of the United States and means by which its preparation may be expedited:* (*a*) *From a highways standpoint*, THOMAS G. MACDONALD, Director, Bureau of Public Roads. (*b*) *From a railroads standpoint*, A. C. BALDWIN, Vice-President, Illinois Central Railroad. (*c*) *From a military standpoint*, Col. C. O. SHERRILL, Corps of Engineers, U. S. Army. (*d*) *Present status of map, and rate and cost of completion*, G. O. SMITH, Director, U. S. Geological Survey, and WILLIAM BOWIE, U. S. Coast and Geodetic Survey. 4. *Extent and means of cooperation between the Board and other agencies:* (*a*) *Federal agencies*, EDWIN F. WENDT, District Engineer, Interstate Commerce Commission. (*b*) *State and municipal agencies*, F. W. DEWOLF, State Geologist of Illinois. (*c*) *Non-Governmental agencies*, ALFRED D. FLINN, Secretary, Engineering Council. 5. *Public needs which a central map information office may serve*, E. B. MATTHEWS, Chairman of Division of Geology and Geography, National Research Council. A general discussion followed the fixed program of the meeting.

International Exchange shipments of publications are now being made direct to Finland in the care of the Delegation of Scientific Societies of Finland, at Helsingfors.

Brood No. 19 of the 13-year race of the periodical cicada ("seventeen-year locust") will be the subject of special observation in Tennessee this spring, to determine the effect of unfavorable weather upon its development. This brood has been subjected twice in its history (in 1894 and 1907) to the unusual condition of freezing weather following its emergence, in May, causing the death of a large proportion of the cicadas before they had begun laying. It is possible that the brood may have been exterminated thereby over a large part of its original territory.

Dr. C. G. ABBOT, of the Astrophysical Observatory, announces that solar radiation measurements at Calama, Chile, have indicated, almost without exception, extremely high values since October 7, 1919. At the same time there has been an unusually severe winter in the northeastern United States and extraordinary cloudiness and precipitation in the Southern Andes.

MR. WALTER M. BERRY, Associate Gas Engineer, has been appointed Chief of the Gas Engineering Section of the Bureau of Standards, succeeding Mr. R. S. MCBRIDE, who recently resigned to join the staff of the McGraw-Hill Company.

Captain W. R. BIRKS, of Chatswood, New South Wales, Australia, visited the Department of Agriculture in February. Captain Birks has been absent from Australia for nearly five years, being connected with the British Army on the western front in France during most of this time. Since the signing of the Armistice he has been studying agricultural conditions in Europe.

Dr. RUPERT BLUE, of the U. S. Public Health Service, went to England in March to represent the United States at the international conference of physicians, surgeons and hygienists, which convened at London on April 12.

Mr. S. R. CAPPS, geologist in the Alaskan division of the U. S. Geological Survey, has applied for furlough and will spend several months in European Turkey studying oil possibilities for the Standard Oil Company. He will be accompanied by Mr. T. P. PENDLETON, topographer in the Alaskan division.

Mr. W. E. CHAMBERS, microscopist and illustrator in the Bureau of Plant Industry, U. S. Department of Agriculture, died on March 5, 1920, in his fifty-fifth year. Mr. Chambers was born at Birmingham, England, in 1866. He had been with the Bureau since August 1, 1908.

Mr. THEODORE CHAPIN, geologist of the U. S. Geological Survey stationed at Anchorage, Alaska, has taken a furlough for four months and will go to the Tampico oil fields, Mexico, for the Standard Oil Company.

Mr. N. H. DARTON, geologist, is on furlough from the U. S. Geological Survey, and will conduct reconnaissance geological surveys for an oil company in northern Mexico.

Prof. HENRY S. GRAVES, for the past ten years Chief Forester of the Forest Service, U. S. Department of Agriculture, resigned from the Service in March, giving as his reason for retirement his belief that "the pecuniary returns afforded professional and scientific men in the Government service inadequately provide against the exhaustion of the working powers which must inevitably take place in time, and entail sacrifices from which employment elsewhere is free."

Mr. PAUL GREELEY has been appointed an assistant at the Calama, Chile, station of the Smithsonian Astrophysical Observatory. He expected to sail from New York about March 20.

Mr. W. B. GREELEY, Assistant Forester, and recently lieutenant colonel in charge of the forestry service of the American Expeditionary Forces in France, has been appointed Chief Forester of the Service, to succeed Prof. H. S. GRAVES on his retirement on May 1.

Dr. RALPH E. HALL, physical chemist at the Geophysical Laboratory, Carnegie Institution of Washington, resigned in March to take charge of physical researches for the Firestone Tire and Rubber Company at Akron, Ohio.

Mr. E. T. HANCOCK has resigned as geologist of the U. S. Geological Survey to represent the Standard Oil Company in Roumania in its oil operations.

Messrs. A. A. HANSEN and F. V. COVILLE, of the Bureau of Plant Industry, and PAUL BARTSCH, of the National Museum, delivered in March and April an illustrated lecture course on "Wild Flowers" under the auspices of the Wild Flower Preservation Society of America in cooperation with the Community Center Department of the Public Schools of the District of Columbia.

Mr. JOHN B. HENDERSON, a regent of the Smithsonian Institution, who for the past two years has devoted the major part of his time to molluscan research in the Division of Marine Invertebrates, has gone to Cuba and Jamaica to secure certain anatomical material of the West Indian operculate landshells necessary to complete a new classification of these mollusks, upon which he and Dr. PAUL BARTSCH are now at work.

Prof. JAMES T. JARDINE, in charge of the Office of Range Research of the U. S. Forest Service for the past thirteen years, resigned from the Service in March to become Director of the Oregon State Agricultural Experiment Station at Corvallis. The work of the office of which he has been in charge embraces a study of the classification, improvement, and use of western range lands, and the period of his administration has seen the development of fundamental principles in range management and their application to 153,000,000 acres of grazing lands in the National Forests.

The following hydrographic and geodetic engineers resigned from the U. S. Coast and Geodetic Survey in January and February: G. R. A. KANTZLER, W. H. OVERSHINER, J. D. POWELL, P. M. TRUEBLOOD, E. M. WILBUR, and S. D. WINSHIP.

Mr. W. S. W. KEW, who has been studying the oil fields of California for the U. S. Geological Survey, will take a furlough for six months to investigate oil fields in Colombia, South America.

Mr. HENRY LINDENKOHL, cartographer of the U. S. Coast and Geodetic Survey, died on February 19, 1920, in his eighty-second year, after fifty-nine years of service with the Survey. Mr. Lindenkohl was born in Hesse-Cassell, Germany, January 26, 1839, and became an American citizen in 1861. He made many contributions to the military maps of the Federal armies during the Civil War, and had been engaged in active cartographic work from that date until the time of his death.

Mr. J. B. NORTON, physiologist in the Bureau of Plant Industry, has resigned to go into commercial plant breeding work at Hartsville, South Carolina.

Mr. ALBERT F. POTTER, Associate Chief of the Forest Service, resigned from the Service in March, requesting that his resignation be made effective April 15.

Mr. E. W. SHAW, geologist, is on leave of absence from the U. S. Geological Survey for six months and will make a reconnaissance of a large tract in Bolivia and the Argentine Republic with a view to its development by an oil syndicate. Messrs. R. H. SARGENT, chief topographer of the Alaskan division, and G. L. HARRINGTON, EDWIN KIRK and C. P. ROSS, geologists of the Survey, also on furlough, will accompany Mr. Shaw.

Mr. EDWARD A. SHERMAN, assistant forester, has been appointed associate forester of the Forest Service, U. S. Department of Agriculture, to succeed Mr. ALBERT F. POTTER, who resigned from the Service in March.

Installments of the large collection of Hawaiian marine mollusks which Mr. D. THAANUM, of Hilo, Hawaii, has donated to the National Museum in order that Dr. DALL's report upon the molluscan fauna of the Hawaiian Islands may be rendered complete, began to arrive in March.

Dr. W. H. WESTON, of the Office of Cereal Investigations, U. S. Department of Agriculture, has completed two years' investigation of downy mildews in the Philippine Islands and will return soon to the United States for conference and preparation of additional papers for publication.

Dr. J. FRANKLIN MEYER, physicist, of the Bureau of Standards, is acting as secretary of the American Engineering Standards Committee, during the absence of the Secretary, Dr. P. G. AGNEW, who is in Europe as a delegate to the International Electrotechnical Commission.